PROCEEDINGS

OF

THE ROYAL SOCIETY.

1832–1833. No. 11.

November 15, 1832.

JOHN WILLIAM LUBBOCK, Esq. M.A., V.P. and Treasurer, in the Chair.

A paper was read, entitled "On some Properties of Numbers in Geometrical Progression." By Charles Blacklewar, Esq. B.A. Communicated by J. G. Children, Esq. Sec. R.S.

This paper contains the demonstrations of the three following theorems; namely,

1°. If the terms of a geometrical series be raised to 2^n, then any odd number of them is divisible by the corresponding terms of the original series.

2°. If each term of a geometrical series be raised to any odd power, the sum of the terms so raised is divisible by the original series, if the number of terms taken be any power of 2.

3°. If the number of terms of a geometrical series be any power of 2, the sum of the terms raised to the power m is divisible by the sum of the same terms raised to the power n, provided m divided by n be a whole number.

November 22, 1832.

JOHN BOSTOCK, M.D. Vice-President, in the Chair.

A paper was read, entitled "Account of an Improvement in the Machine for producing Engravings of Medals, Busts, &c. directly from the Objects themselves, in which the Distortions hitherto attending such Representations are entirely obviated." By Mr. Bate. Communicated by J. G. Children, Esq. Sec. R.S.

Some printed representations of medals having been received from America, about fifteen months ago, evidently effected by some process of ruling, Mr. Bate, jun. constructed an instrument for accomplishing the same object; but the results, both of the American method and of the one invented by Mr. Bate, were attended with a degree of distortion. This the author has ingeniously obviated, by giving an inclination of 45 degrees to the plane in which the tracing-line is moved over the surface of the object of which a representation is to be given.

A paper was also read, entitled "An Account of the Construction of a fluid refracting Telescope of eight inches aperture and eight feet nine inches in length, made for the Royal Society by George Dollond, Esq. F.R.S." By Peter Barlow, Esq. F.R.S.

The author has, in former papers read to this Society, pointed out the great variety of cases included under the general formulæ relating to the operation of fluid refracting telescopes, and stated the difficulty of selecting, independently of experiment, the particular case which was likely to produce the best result. This subject is pursued in the present paper; and the principles and calculations stated at length which the author has applied in the construction of the telescope which the Council of the Royal Society directed should be made by Mr. Dollond, under the superintendence of the author, in order to put these principles to the test of experiment, and to decide the question of the expediency of proceeding in the construction of a similar telescope of much larger dimensions. When the experimental telescope was completed, it was found that its performance agreed in every respect with the computed results, as well in focal distance as in chromatic and spherical aberration. The arrangement of the lenses was such, that the corrections are all of them made in the transmission of the light through the fluid, and by the fluid only. The author abstains from offering any remarks on the performance of this telescope, leaving it to those whom the Council of the Royal Society may appoint, to decide upon its merits. He concludes by expressing his obligations to Mr. Dollond, for the readiness with which he complied with all the suggestions of the author, and for the accuracy with which he has executed every part of the instrument.

Anniversary Meeting, Nov. 30th.

HIS ROYAL HIGHNESS THE DUKE OF SUSSEX, K.G., President, in the Chair.

The President delivered the following Address:

GENTLEMEN,

THIS is the Second Anniversary of my election to the Chair of the Royal Society, and I gladly avail myself of the opportunity which it affords me of renewing the expression of my gratitude to you for the distinguished honour conferred upon me in electing me to it, and still more for the continued kindness and support which I have received from you in the execution of the duties of my office. I can only assure you, Gentlemen, that if it be your pleasure that I should continue to fill this Chair, I shall feel an additional motive to induce me to devote my most earnest attention to the promotion of the interests of the Society, in the perfect reliance which I place upon your cooperation and assistance, and in the confident expectation which I entertain, that in case I should fail in the due and efficient discharge of any of my duties, I shall experience from you the most kind and liberal interpretation of my motives and conduct.

In making my acknowledgments to the Fellows of the Society at

large for their uniform kindness and support, it would be injustice and ingratitude on my part were I not to return my public and especial thanks to the Vice Presidents, Treasurer, Secretaries, and the other Members of the Council—

To the Vice Presidents, as well for their general services as also for their kindness in supplying my place in this Chair, when I have unfortunately been compelled to be absent from the state of my health, or from the immediate necessity of discharging other most pressing public duties.

To the Treasurer, for his vigilant attention to the finances of the Society, and to every arrangement which may in any manner tend to promote the usefulness of the Institution, and increase the accommodation of its Members.

To the Secretaries, for their courteous discharge of their various and very laborious duties: and to the Members of the Council collectively, for their regular and punctual attendance at all the meetings to which they have been summoned, and for the zeal and readiness with which they have undertaken any labour, however considerable, which the interests of the Society might require them to perform.

The Report of the Council which will be read to you by one of your Secretaries, Dr. Roget, will make known to you various matters connected with the administration of the Society, and also the arrangements adopted for supplying the deficiencies of the Library in different departments of science, and for rendering it more generally accessible, and therefore more useful, by means of complete and well classed catalogues. I must refer you likewise to the same Report for a statement of the grounds upon which two Copley Medals have this year been adjudged, one to Mr. Faraday, and the other to Mons. Poisson. There is, however, one arrangement, admirably calculated, in my opinion, to increase the usefulness and to uphold the credit of the Royal Society, which that Report does not notice; I mean the Resolution adopted by the Council to allow no Paper to be printed in the Transactions of the Royal Society, unless a written Report of its fitness shall have been previously made by one or more Members of the Council, to whom it shall have been especially referred for examination. This Resolution has been acted upon for the greatest part of the last year, and some of those Reports of a favourable nature have been read before the Society, and printed in the Abstracts of our Proceedings. When the number of papers which come before the Society in the course of a year is considered, as well as the great diversity and occasional difficulty of the subjects which they embrace, it will be at once seen how greatly the labours and responsibility of the Members of the Council must necessarily be increased by the rigorous adoption of such a system. It is in consequence of the important influence which this plan is likely to have upon the well-being of the Society, that I am induced to enter somewhat in detail into the reasons which have led to its adoption.

It has long been the custom of many Foreign Societies, and particularly of the Academies of Science and of Medicine at Paris, to require written Reports upon every paper submitted to them,

from a Committee of their Members: as the persons who are selected for this duty are frequently veterans in their respective sciences, who have earned by their labours an European reputation, the Reports which are thus produced prove often more valuable than the original communications upon which they are founded, and the collections of them, as is well known, form a most important part of the stock of modern science. Many other advantages also have been found to result from the adoption of this practice. The decisions of men who are elevated by their character and reputation above the influence of personal feelings of rivalry or petty jealousy, possess an authority sufficient to establish at once the full importance of a discovery, to fix its relations to the existing mass of knowledge, and to define its probable effect upon the future progress of science. They thus operate as a powerful stimulus to the exertions of the genuine cultivators and lovers of science, who feel assured that their labours will be properly examined and appreciated by those who are most competent to judge of their value; whilst, at the same time, they tend to keep under the obtrusive and turbulent pretensions of those who presume to claim a rank as men of science, for which they possess no just title or qualification.

It was from a conviction that many similar advantages would result from such a system of Reports in the Royal Society, that the Members of the Council were induced to agree to its adoption; and it is to be hoped that, when a longer experience has given to such a plan a more complete organization, and has shown the practical extent to which it can be conveniently carried, it will then become a permanent law of the Council.

In order, however, to secure its full advantages, it will be necessary that the Council should, in all cases, include men eminent for their proficiency in all those branches of science which usually come, or are likely to come, under the notice of the Society. That such men may be found, I feel satisfied, both from my past experience and from my knowledge of the many distinguished persons who adorn the lists of this Society; and that such men would generally be ready to undertake the performance of a duty, requiring the occasional sacrifice both of time and labour, I cannot venture to doubt, without imputing to them a charge of indifference to the interests and the usefulness of the Royal Society, and even a want of proper sympathy with the scientific honour of their country.

I think myself justified in using such strong language, Gentlemen, because I believe the scientific character of this country to be most intimately associated with the scientific character and estimation of the Royal Society. One of the most illustrious of modern mathematicians and philosophers, himself a foreigner, has said that the Royal Society has contributed more to the progress of science than the combined labours of all other similar institutions; and though it would be unfair to interpret too literally the language of a compliment, yet it would not be very difficult to vindicate its general truth and justice.

It was this Society which fostered and encouraged the early labours

of Newton, and under its auspices was published the work which constitutes, and probably ever will constitute, the proudest monument of the genius of man: and from the period which immediately followed its foundation, the age of Wallis, and Newton, and Wren, and Hook, and Halley, and Taylor, to that of Herschel and Cavendish, and Wollaston, and Young, and Davy, its Transactions contain records of almost every important discovery in natural philosophy; of almost every experimental inquiry which has been most remarkable for its difficulty, delicacy, or importance; and of almost every original speculation which has most contributed to the advancement of science.

It becomes us therefore to guard these national archives of the progress of knowledge, with the reverence which is due to them as monuments inseparably connected with our own national honour; and to watch with our utmost care, lest any addition should be made to them, which can be considered as unworthy of the character of the stock upon which it thus becomes engrafted; and it therefore is the bounden duty of every Fellow of this Society, whether it be considered as imposed on him by the terms of the Obligation which he signed at the period of his admission as a Member, or as derived from the still higher and more comprehensive ties which bind every friend of the great institutions of his country, to maintain their efficiency and credit, and to allow no private or personal cause of jealousy or discontent, no trivial or unfounded plea of want of leisure from business, or occupations, to interfere with the devotion of his best exertions to uphold the character and promote the interests of the Royal Society.

There are some reasons which I know may, and very probably will be urged against the reasonableness of expecting that any considerable number of men of science, should be able, however willing they might otherwise be, to devote any large portion of time or labour to the service of any Society, let its claims upon them be ever so strong.

In this country, where wealth is the general measure of the social rank of families at least, if not of individuals, men of science must either possess an independent fortune, or they must pursue it, as is most generally the case, in connexion with a laborious profession; for we have few establishments which afford them support, independently of other employments; and even in those very rare cases, the provision which is made is so small, that no man of superior education can look forward to the attainment of the advantages which science and learning offer, in forming his scheme of life, unless he be prepared to make the most serious sacrifices. It is for this reason, that the learned professions, presenting as they do the most brilliant prospects of rank and wealth, generally absorb, in the progress of life, the studies and exertions of young men of the highest scientific education and promise; for, however strong may have been their attachment to the studies of their youth, and however ardent their ambition to obtain the honours of science, they soon find that such pursuits retard their professional advancement.

In other countries, however, where the learned professions are neither richly paid nor highly honoured, and where the exclusive cultivation of particular branches of literature and science presents the readiest access to the possession of competence and social rank, we find large bodies of men who have no professional engagements whatever to divert them from their literary and scientific labours, which are thus made to constitute the business of their lives. I am fully sensible of the great advantages which other countries possess in these respects above our own, and that it is quite impossible for us to command an equal concentration of attention to the advancement of particular branches of science, or to the concerns of a particular Society; still less so when it is considered, that those services must with us be afforded gratuitously, which in other countries are remunerated by the State, or are required as part of the duty of a salaried office:—we are not less called upon, however, on this account, to make the best and most efficient use of the means in our power, and the assistance which we cannot command as due from a sense of official or professional obligation, we may receive as rendered from a higher feeling of devotion to the promotion of the general interests of science, and with it of our national fame.

However much I may lament the want of establishments, in this country, for the exclusive and liberal support of men of learning and of science, and however anxiously I may look forward to the time when our Government and Legislature may take this subject into their most serious consideration, with a view to the remedy of so great an evil, yet I rejoice to observe amongst all ranks of society so zealous and so ardent a feeling in favour of the cultivation of every branch of science, of art, and of literature; so general and so deep an anxiety, in fact, that our country should advance in the front rank in the rapid march which European nations are making in knowledge and improvement.

It would be very easy for me to produce evidence of the existence of this spirit in the foundation of literary and other Societies in so many of our provincial towns, and in the active and general support which they receive; but it is sufficient for my purpose to appeal, for the complete confirmation of the truth of the opinion which I have expressed, to the noble manner in which the British Association has been supported, by the eager concurrence of the friends of science from all quarters of the kingdom: and the splendid reception which has been recently given to this Association by the University of Oxford; the judicious and well merited honours conferred upon four of its most illustrious Members*; the eager attention which was given to its proceedings by crowds of intelligent and admiring auditors, the great variety and excellence of the Reports which were there produced upon the present state and recent history of various branches of philosophy, will constitute a proud epoch in the scientific history of this country, and one which is full

* Brewster, Brown, Dalton and Faraday, on whom the degree of LL.D. was severally conferred.

of promise with respect to the future state and fortunes both of science and its cultivators.

It becomes my duty now to advert to the heavy and severe losses which the Society has sustained during the last year, including, I regret to say, many celebrated names, more particularly in our foreign list. I shall begin, however, with the mention of those names upon our home list, whose labours in the cause of literature or of science, appear to entitle them to particular notice.

Sir Everard Home, Bart., was born at Hull on the 6th of May 1756. He was the youngest son of Robert Home, a surgeon in the army, and descended from the Barons of Polwarth, the ancestors of the Earls of Marchmont in Scotland: he was educated at Westminster School, and though elected off as a scholar to Trinity College, Cambridge, in 1773, he never went there, having abandoned his prospects in college upon the invitation of the celebrated John Hunter, who had recently married his eldest sister, and who offered to superintend his education in surgery and human and comparative anatomy, and gave him the free use of his unrivalled collections. Under his auspices he continued to study for several years, availing himself at the same time of the lectures and instructions of the most eminent anatomical and medical teachers of his day. He went to the West Indies upon the medical staff in 1780, where he remained for four years; upon his return to England in 1784, he continued to assist Mr. Hunter in the arrangement and completion of his museum, and also in his various official duties until his death, which took place in 1793. Mr. Home was elected a Fellow of this Society in 1785; in 1808 he was made sergeant-surgeon to the King, and in the same year he received the Copley Medal for his various papers on Anatomy and Physiology, printed in the Philosophical Transactions. In 1812 he was created a baronet, being the first surgeon in actual practice upon whom that honour had been conferred.

In 1821 he was appointed surgeon to Chelsea Hospital, and in the following year he was elected President of the College of Surgeons. In the year 1827 he began to retire from the practice of his profession, and from most of his official employments; and he died at his residence in Chelsea College in August last, in the 77th year of his age.

Sir Everard Home was the author of 107 papers in the Transactions of this Society, a number exceeding that of any other contributor. He published Lectures upon Comparative Anatomy, in six volumes quarto; the two first in 1814, the third and fourth in 1823, and the two last in 1828. They consist chiefly of the results of his papers in the Transactions of this Society, with a republication of the splendid plates, by the permission of the Society, by which many of them were illustrated. He was also the author of several other works upon different subjects of anatomy and surgery; and he published in 1797, Memoirs of John Hunter, who had bequeathed to him all his papers.

Sir Everard Home must be considered as the successor of John Hunter, and in every way most closely connected with him. He

aided greatly in the formation of his noble collection; he was a witness of, and a sharer in, his most important investigations; he was also the depository of his literary treasures; and if we regard either the number or the nature of his anatomical or physiological researches, and the importance of his discoveries, we must be compelled to declare that he followed closely and worthily in the footsteps of his illustrious predecessor: but though he was a most diligent observer and collector of facts, and fully qualified, by his extensive knowledge of anatomy and physiology, to collate them with existing materials of those sciences, and to reduce them, as he has done in his lectures, to a regular and well-connected system, yet we should be unjust to the memory of that great man who was his instructor and patron, if we ventured to place him in the same rank with him. But what name in modern times, if that of Cuvier be excepted, can be put in competition with that of John Hunter, for careful and philosophical induction, and for the power of concentrating facts derived from most extensive observations upon every part of the animal kingdom, in illustration and confirmation of his physiological theories? It would be unfair to the memory of Sir Everard Home to subject his merits and his fame to be tried by so severe a test; rather let us ask, when the vast range of his knowledge and investigations is considered, who were his rivals or his superiors among his contemporaries, or amongst his survivors?

Sir James Hall, Bart., the author of several important papers in the Edinburgh Transactions, in illustration and in defence of the Huttonian Theory, and of a very ingenious and speculative book on the Origin of Gothic Architecture, is another considerable name, whose loss we have to deplore.

In considering the present state of geological science, we are too apt to forget the fluctuations of opinions and of theories through which we have passed in order to arrive at our present state of comparative repose. It is little more than twenty years since the partisans of Hutton and of Werner divided between them the geological world, and we rarely hear their names now pronounced; not that their names have passed into oblivion, but that their theories and their speculations have become a portion of the history of the science, and no longer form a part of the debateable materials of which it was, or was not, to be constructed. Sir James Hall, in conjunction with his friend Professor Playfair, was, in the early part of the present century, an ardent vindicator of the opinions of Dr. Hutton; and it was with a view to the removal of some of the more popular and startling objections to his theory, that he undertook, and continued during several years, those memorable experiments upon the effects of compression in modifying the action of heat, which have contributed so greatly to the termination of the controversies which were then agitated with so much warmth and severity. These experiments, most happily conceived, and executed with singular boldness and perseverance, completely proved that the most refractory substances may be made fusible by confining the elasticity of the gaseous parts contained in them. Thus, pounded carbonate of

lime or chalk could be rendered fusible, without calcination, and became, upon cooling, a compact stony mass, and even crystalline, like marble: it thus appeared that the effect of heat, acting under enormous pressures, would not necessarily dissipate the gaseous and evaporable parts of the strata of the earth, but would leave them to form such new combinations or modifications of existence as might be determined by the laws of crystallization or of chemical affinities;—a most important fact, and one apparently so difficult to establish in a form which might bring into action those gigantic forces which present themselves in the great operations of nature, as would have checked the attempts of any man who was not urged onward by the most determined enthusiasm in the defence of a favourite theory.

Sir James Hall's work on the Origin of Gothic Architecture cannot be considered as a serious archæological inquiry, but rather as an agreeable exercise of his fancy. The development however of his theory is singularly ingenious and elegant; it proves him to have possessed no mean talents as an artist, and shows a mind alive to all those beautiful combinations of nature which seem to be rendered fixed and permanent in the naves of our Gothic cathedrals, and in the tracery of our decorated windows.

Sir James Mackintosh was born in Morayshire in Scotland, in 1765; he was the son of an officer, of good family, but of very limited fortune; his first destination was for the profession of medicine, and with this view he took the degree of M.D. at Edinburgh, in 1787. Upon his removal, however, to London, shortly afterwards, he abandoned his medical prospects, and gave himself up entirely to the study of the law, and of moral and political philosophy. In 1789 he went to Leyden, where he studied for some time, and afterwards to Liege, where he was a witness of the memorable struggle between the Prince Bishop and his subjects, as well as of many other ebullitions of popular feelings which preceded and foreboded the French Revolution. It was, probably, the contemplation of scenes like these, as well as the observation of the corruptions and abuses of many of the continental governments of Europe, which made him, like many other ardent young men of that period, an admirer of the principles of that great national movement; and the *Vindiciæ Gallicæ*, a work of great force and eloquence, was the most powerful answer which appeared in that age to Mr. Burke's celebrated Reflections, and gained for him, at once, both at home and abroad, a distinguished reputation. The atrocities, however, which marked the more advanced stages of the French Revolution, his own increasing experience and knowledge of mankind, and still more his frequent intercourse with his illustrious adversary, for whose genius he had always professed a chivalrous admiration, however much he had opposed his views and his reasonings, combined to sober down the fervent enthusiasm of his own youthful speculations and hopes; and the principles which he avowed and vindicated in his celebrated defence of Peltier in 1802, must be considered as those which he adopted as the result of the convictions of his maturer age, and

which he continued to maintain through life. In 1803 he was appointed Recorder of Bombay, where he resided for seven years, and where he secured the affection and admiration both of natives and of foreigners, by the able, impartial, and considerate discharge of his judicial functions. Upon his return from India in 1811, he was elected Member of Parliament for Knaresborough, a place which he continued to represent for the remainder of his life.

Few persons of his own age had read so much as Sir James Mackintosh, or remembered what they had read so well. His conversation was singularly instructive and brilliant, without being overbearing; his manners were conciliating, his temper excellent, and he was entirely tolerant of opinions which were different from his own. He was one of the most distinguished Members of the House of Commons; and his speeches upon all the great questions which were agitated in his time were remarkable, not merely for their eloquence, but the large and comprehensive views of national policy, which were supplied by his almost unrivalled knowledge of history and political philosophy.

Sir James Mackintosh, besides his *Vindiciæ Gallicæ*, was the author of Lectures upon the Laws of Nations; of A Sketch of the History of England; of an incomplete Essay on the Principles and the History of Moral Philosophy; and of many admirable Reviews. It is to be lamented that he should have dissipated his extraordinary powers upon occasional and desultory publications, instead of concentrating them upon some great work, which might have transmitted, undiminished, to posterity the reputation which he enjoyed among his friends and cotemporaries. There were, however, many circumstances which might sufficiently account for his failing to leave behind him a monument for future ages, which would have been worthy of his genius and his learning. He brought home with him from India a shattered constitution, which disqualified him for continued and laborious exertion; he had many Parliamentary as well as official duties to perform; and the pressure of his pecuniary necessities compelled him to seek, too frequently, for the immediate remuneration which was supplied by means of contributions to the perishable periodical literature of the day.

Colonel Mark Wilks went to Bengal in 1783, and served in different military and civil capacities in various parts of India. In the year 1804 he was appointed principal Resident at the Court of Mysore, and in the following year he published a very able Report upon the financial condition, resources, and many other subjects connected with the administration of the government of that country. He was the author of "Historical Sketches of the South of India, in an attempt to trace the History of Mysore to the Extinction of the Mohammedan Dynasty in 1799,"—a work of great learning and authority: he was afterwards appointed Governor of St. Helena, and he died in England in the course of the present year.

Colonel Wilks must be considered as one of those distinguished men who have been formed by the system of our Indian Empire. The possession of great commands, upon which the happiness and

misery of considerable nations are dependent, and the intense feeling of responsibility which is connected with the administration of trusts so important, is well calculated, under all circumstances, to call forth into action the highest powers of the human mind; and particularly so, when they have been previously exercised and fortified, as in our Indian service, by the severe study of Oriental languages, and by the successive occupation of different offices, with a great diversity of duties: it is to such causes that we are to attribute the frequent union which we observe in this service of the greatest civil and military talents with the most profound acquisitions in Oriental learning; it is to this system that we are indebted for the production of a Duncan and a Monro, an Elphinstone and a Raffles, a Colebrooke and a Malcolm, and a crowd of great men who have done so much honour to our Indian Government.

Alexander Barry, Professor of Chemistry to Guy's Hospital, and the author of a short paper in our Transactions for 1831, "On the Chemical Action of Atmospheric Electricity," fell a victim to the imprudent pursuit of his chemical inquiries. He was making experiments upon some gases in a highly condensed state, when an explosion took place, by the effects of which he was so much injured as to occasion his death shortly afterwards. He was elected a Fellow of this Society in the course of the last year.

John Shaw, Architect, is advantageously known to the public by several works in the Metropolis, particularly the great hall in Christ's Hospital, and the new church of St. Dunstan in Fleet Street: works which are extremely effective, and well adapted to their objects and positions.

Stephen Groombridge, Esq., was the author of two papers in our Transactions for 1810 and 1814, of considerable interest and value, upon the subject of astronomical refractions: his observations were made at his house at Blackheath, with a four-feet transit circle, which has acquired no small degree of celebrity from its being the first instrument, after the Westbury Circle, to which Mr. Troughton applied his method of division, which he has described in our Transactions. Mr. Groombridge made many thousand observations, which have been reduced by order, and published at the expense of, Government,—a circumstance well deserving to be known by all astronomers, as he was an able and faithful observer, and possessed more advantages for making meridian observations than are commonly enjoyed without the walls of a regular observatory.

Sir Richard Hussey Bickerton was a very distinguished naval officer, who was employed in the service of his country for the greatest part of his life, and who was for some time second in command to Lord Nelson in the Mediterranean and elsewhere, and enjoyed his entire confidence and esteem. He was one of the Lords of the Admiralty from 1805 to 1812, a circumstance which brought him into frequent communication with the Royal Society, and led to his election as a Fellow in 1810.

In our list of Foreign Members, we have to record the deaths of Cuvier and of Chaptal in France, of the Baron de Zach in Germany,

and very lately likewise those of Oriani and of Scarpa in Italy; five celebrated names, which have long been intimately associated with the progress of science. The limits of this address must confine me to a very brief and imperfect notice of their merits and their labours.

The Baron Cuvier, the most illustrious naturalist of modern times, was born at Montbelliard in Alsace, in 1769, and died on the 13th of May last, in the 63rd year of his age: it is not necessary for me to detail any of the circumstances of the life of one whose name has been long known and reverenced in every region of the globe which has enjoyed the blessings of European civilization; suffice it to say, that he was honoured and even courted by every Government in France from the period of the Convention to the present day; that he held the most lucrative and distinguished appointments which the wise policy of that great nation has provided for the honourable support of its men of science and literature; that after the death of Laplace he was universally regarded by his countrymen as the most illustrious of their men of science, and as one of the most distinguished of their men of literature; that funeral orations were pronounced over his grave by men of all political parties, however much opposed to him during his life; and mathematicians and naturalists, geologists, historians and poets, all felt themselves impelled to pay this last tribute of homage to the genius of one, who in so many capacities had done so much honour to his country.

M. Cuvier was in every respect a most extraordinary man: his very presence was calculated to command respect, his countenance bearing that impress of a powerful intellect, which all men recognise when seen, however difficult it may be to define its character: his manners were dignified and polished, and his conversation possessed that happy ease and subdued gaiety which characterized the best age of French society. He was well acquainted with ancient literature, and familiar with the principal languages of modern Europe. His memory was singularly accurate and retentive; and his knowledge of facts, not merely in those sciences which he especially cultivated, but likewise in all other departments of knowledge, and particularly history, was a subject of surprise and admiration to all who knew him. He was also eminently distinguished as a writer of his own language, and his numerous *éloges* delivered in his capacity of *secrétaire perpetuel* to the Institut, of which three volumes have been published, if considered as specimens of composition merely, have equalled, if not surpassed, the best examples of a species of eloquence of which the French nation has just reason to be proud; but if they be considered as specimens of correct and precise discrimination of the merits of the persons commemorated, as determined by their writings and discoveries, and by the influence which they have exercised upon the progress of knowledge, they may justly be pronounced to be unrivalled. It was to this publication that he was indebted for his place amongst the *forty* of the *Académie Française*, an honour which he alone, in his own age, enjoyed in conjunction with his place in the *Académie des Sciences*.

It is, however, chiefly as a naturalist that Cuvier must be viewed, when we seek to determine his permanent rank amongst the few great men who have effected great revolutions in the sciences which they have cultivated, or have left ineffaceable traces of the influence of their discoveries behind them. The whole animal kingdom, from the most obscure indications of the separation between inanimate and animate existence to the mighty monsters of a former world, has assumed under his hands a systematic arrangement, not founded upon superficial and unimportant external characters merely, but upon a most careful and laborious observation of the analogies of internal structure. By tracing every organ successively through the whole series of animals; by carefully determining the functions of such organs and their relations to each other; and by considering them in every animal in the first place as an individual, and in the second place with reference to others, he has been enabled to distribute them into species and genera, and families and classes, where every successive step in their arrangement is the result of a legitimate and inductive generalization. It is by such means that he has been enabled to convert the science of natural history, at least in the animal kingdom, from being little more than a systematic classification, formed for the purpose of identifying genera and species and with no higher view, into a science of strict and severe induction, founded upon a careful observation and comparison of every fact which anatomical and physiological science can detect, and thus to confer upon it a dignity which is only inferior to that of the physical sciences.

It has resulted also, from his researches, that every animal considered as one of the same genus or species, is not only an individual considered as a whole, but also when considered in all its parts; in other words, that every bone, every muscle, every organ, and every part of its structure is *essentially* distinguished from the corresponding parts of an individual of any other genus or species. To a perfect naturalist, therefore, the inspection of a bone, or any other part of an animal, would bring to his mind the entire animal itself, and would identify it as perfectly as if it was exhibited entire to his eye: this would be a triumph of science to which our limited knowledge and faculties can never completely attain; but it was to this point that Cuvier approximated, when he reconstructed as it were the fossil animals of an antediluvian world from the imperfect fragments which remained of them; when he showed in what such animals must have differed, and in what they must have agreed, whether in magnitude or in kind, from the animals which exist at present; when he ventured in fact to define their habits, and to write as it were the natural history of a former world, by throwing upon its obscure and half-obliterated records the powerful light of science and philosophy. The *Histoire des Ossemens fossiles* must ever remain a classical work to geologists; and the discoveries which it contains, and those to which it has led in the hands of others, are some of the most interesting and extraordinary with respect to the past ages of the world, which ob-

servations upon the surface of the globe have ever enabled us to ascertain.

The last great work upon which he was engaged was the *Histoire Naturelle des Poissons*, a prodigious undertaking, of which eight volumes have been published, and which he expected to extend to twenty-five: it was undertaken in conjunction with Messrs. Valenciennes and Laurillard, to whom also he has bequeathed the task of completing it. It will contain the description of 6000 species of fish, 4000 of which had not been noticed in any other work.

Jean Antoine Chaptal, Comte de Chanteloup, was born in 1756, and died in April last in the 76th year of his age. He was Professor of Chemistry at Montpelier before the Revolution, and was one of the most active cultivators of chemical science before that event, in conjunction with Monge, Fourcroy, Berthollet, Guyton de Morveau, and the illustrious Lavoisier. In the year 1793, upon the threatened invasion of France by the Allies, when saltpetre was not to be procured in sufficient quantities for the manufacture of the powder wanted by the French armies, he was invited by the Committee of Public Safety to superintend the establishments for that purpose; and his chemical knowledge so greatly improved the method followed in its manufacture, as in a very short time to make the produce greatly exceed the demand. He was made *Ministre de l'Intérieur* by Napoleon, and continued under the Empire to fill many important situations. He was the author of considerable works on chemistry, on the application of chemistry to the arts, on the application of chemistry to agriculture, on the art of making wines, and on the art of dyeing cotton and wool, which are written in a very perspicuous and elegant style, and which have enjoyed a very considerable popularity in France. The labours of his whole life, in fact, were devoted to the improvement of those manufactures whose perfection depended more or less upon the most correct and economical application of chemical principles; and, after his distinguished countryman Berthollet, he must be placed in the first rank of those who have benefited the arts through the medium of chemical science.

François Xavier Baron de Zach was born at Pesth, in Hungary, in 1754. His taste for astronomy was decided at the early age of fifteen, by the interest which he took in the observation of the comet of 1769, and by the transit of Venus over the disc of the sun in the same year, a memorable event which served to make more than one important convert to the science of astronomy. After travelling with scientific views through different countries of Europe, and residing for several years in England, where he acquired for our manners and institutions an attachment which continued throughout his life, he settled at Gotha, in 1786, in the family of the Duke of Saxe Gotha, who charged him with the construction of the Observatory at Seeberg, over which he continued to preside for a considerable period. He published at Gotha, in 1792, Tables of the Sun, with a Catalogue of 381 Stars, and he subsequently published many other

important astronomical Tables, particularly those on Aberration and Nutation. He became in 1800 the editor of the "Monatliche Correspondentz," a German periodical work on astronomy and geography, which was subsequently published in French under the title of "Correspondance Astronomique &c.," upon his removal to the South of France in 1813, and subsequently to Genoa in company with the Duchesse de Saxe Gotha. This was a most valuable Journal, containing records of the progress of astronomy in every country in Europe, and contributing more than any other publication to the great impulse which has been given for many years to the cultivation of astronomical science in Germany. In 1814 he published his very interesting work on the "Attraction of Mountains." For many of the later years of his life he suffered severely from the stone, and he had established himself at Paris for the purpose of being constantly under the care of Dr. Civiale and experiencing relief by the operation of lithotrity, where he died from a sudden attack of cholera in September last. The Baron de Zach was a most zealous friend to astronomy, and throughout his long life contributed to its progress by his numerous publications, and by maintaining a most extensive and laborious correspondence with the principal astronomers in Europe. He was a man of warm and ardent affections, rapid and sometimes hasty in his conclusions, of the most lively and agreeable manners, and of the most indefatigable industry: and there are few persons of the present day whose loss will be more sensibly felt by the friends of astronomical science in every country in Europe.

Barnaba Oriani, Director of the Observatory of the Brera at Milan, where he has resided for fifty-five years as assistant and principal, was born at Garegnano near Milan, in 1753. He was the principal conductor of the measurement of an arc of the meridian in Italy, and of the great trigonometrical survey of Lombardy, which took place between the years 1786 and 1790; and throughout the course of a long life, he devoted himself to the cultivation of physical and practical astronomy. He was the first person who calculated the orbit of the planet Ceres after its discovery by Piazzi at Palermo. He published theories of the planets Uranus and Mercury, with Tables of their motions. He laboured with singular skill and perseverance in the improvement of the lunar Tables both by theory and observation. He was the author of an admirable treatise on spheroidical trigonometry: and the Astronomical Ephemeris of Milan was published for many years under his directions, by Carlini. Upon the whole, if the union of practical with theoretical science be considered, we shall be justified in pronouncing him to have been, after Bessel, the most accomplished astronomer of the present age.

Antonio Scarpa, one of the eight Foreign Members of the *Académie des Sciences* of Paris, and probably the most profound anatomist of the present age, was born in the year 1746, and died in October last in his eighty-seventh year. He was made Professor

of Anatomy at Pavia in the twenty-second year of his age, and for the last half-century he has been placed by the common consent of his countrymen at the head of their anatomists and surgeons. He was the author of magnificent and classical works on "The Organs of Hearing and Smell," "On the Nerves," "On the principal Diseases of the Eye," "On Aneurism," "On Hernia," with Memoirs on many other subjects of physiology and practical surgery. He had accumulated a handsome fortune by the practice of his profession, and had collected in his palace at Pavia a considerable number of works of art, where he lived for the latter years of his life, surrounded by his pupils, reverenced by his countrymen, and in the enjoyment and contemplation of that brilliant reputation, the full development of which a great man can rarely live to witness.

In thus directing your attention, Gentlemen, to those distinguished Members of the Royal Society, who, unhappily for the interests of science, have been taken from us during the last year, there is one name remaining which I cannot notice without feelings of the most painful embarrassment. To what class shall I, or can I refer it; to the living or to the dead? Though my fears tend too strongly to make me decide upon the choice of the latter, yet I would fain indulge in the hope which is still afforded by the uncertainty, mournful though it be, which hangs over the fate of the gallant and adventurous Captain Ross. The object of his voyage, as is well known to you, was the solution of a nautical problem of the greatest interest and difficulty,—the discovery of a north-west passage. It is a problem which more than any other excited and baffled the adventurous spirit of our most daring seamen of the age of Elizabeth; and when subsequently resumed, chiefly upon the authority of the ingenious speculations of Daines Barrington, a distinguished Member of this Society, and of others of later date, the first attempt of Captain Ross himself and the memorable voyage of Parry, as well as the journey of Franklin, have shown how visionary were all hopes of its successful solution for the purposes of commerce, however interesting it might be for those of science. It was the failure of the first voyage of Captain Ross, and the apparent censure which he conceived rested upon him, in consequence of the greater success of the attempt of his immediate successor in this enterprise, which oppressed his high and manly spirit, and made him seek, with the greatest possible earnestness, for an opportunity of vindicating his professional character. With the assistance of some of his friends he planned another voyage, and nearly three years ago he proceeded to put it into execution. It is to dispel the mystery attendant upon that voyage, of which no tidings have been yet received, and to relieve the misery under which the friends and relations of Captain Ross and his gallant crew are lingering, that a vessel is now preparing, under the command of an able and experienced officer, to pursue the track which he probably followed. I have consented, at the request of the Royal Geographical Society, to be placed at the head of the Committee which has been formed for the aid and

furtherance of this benevolent plan, and I confidently hope that the funds which are necessary to complete this undertaking will not be found wanting.

The name of nearly every distinguished foreigner who has been lost to science during the last year has appeared upon the Foreign List of the Royal Society, and I cannot help considering it as a circumstance which does honour to the Royal Society, that it should thus have associated with it whoever is most eminent in the great aristocracy of European science. It is my wish, Gentlemen, and I feel assured that it is yours also, that the Royal Society should embrace the name of every distinguished man of science in the British dominions. At the last Anniversary it was my pleasing duty to present the Copley Medal to Professor Airy,—a name which would do honour to any Society, but which does not yet appear in the list of our Members: and I lament that I am not allowed to commemorate the name of that very distinguished philosopher, Sir John Leslie, upon this occasion in the obituary of the Royal Society. I look forward with hope, Gentlemen, to the time when the Royal Society shall be so circumstanced as to be free from such a reproach, or rather from such a misfortune.

Report of the Council to the Anniversary Meeting on St. Andrew's Day, 1832.

The Council of the Royal Society have, during the past year, used their most earnest endeavours to render the Library as effective for the purposes of science, as the means at their disposal would enable them. They have been desirous, in particular, to make it as complete as possible in all those departments of science, which it is more especially the object of the Royal Society to cultivate and to advance. They have accordingly purchased, with the advice of the Library Committee, such books as were more immediately required for these purposes, at an expense of about £1600. It was evident, however, that the mere possession of these books by the Society would be of little avail to those who wished to use them, until they were arranged and catalogued according to some uniform and well-digested method. A Committee was therefore appointed to consider of the best plan of effecting this desirable object; and to suggest measures for obtaining a correct catalogue of the library, arranged under such specific heads as were best calculated to assist the inquiries of all those who might resort to it for information. Various plans for this purpose were proposed and discussed: and it was finally determined that in order to insure uniformity of execution, the whole labour of compiling the new classed Catalogue, and of conducting it through the press, should be confided, though still under the superintendence of the Committee, to one person only; provided a proper person could be found who was fully competent to so arduous a task, and also willing to undertake it. The Council have accordingly engaged Mr. Panizzi, of the British Museum, a gentleman of great literary attainments, and conversant with that kind of labour, to undertake

this charge; and have no doubt that he will accomplish it to the full satisfaction of the Fellows of the Society at large, to whom the possession of such a classed Catalogue as the one proposed, will be advantageous in many ways, independently of its direct utility in reference to the employment of the library.

The whole of the sum at which the Arundel Manuscripts which have been exchanged for books, were valued, has now been received from the Trustees of the British Museum, and the account with them is thereby closed.

The Council have also directed the printing of an edition of the Abstracts made by the Secretaries and entered on the Journal Book of the Society, of such papers as have been read to the Society and ordered for publication in their Transactions, from the year 1800 inclusive, to the present time. They conceive that a collection of these Abstracts, which possess in themselves much intrinsic value, will form an useful sequel to the Abridgement of the Philosophical Transactions of which the public is already in possession, but which does not extend to a later period than the end of the last century. This work will form two thick octavo volumes, one of which is now completed and ready for delivery to subscribers. The proof sheets, at the desire of the Council, were read over by Mr. Lubbock and Mr. Children, and no alterations were made except for the correction of errors obviously arising from inaccurate transcription. The Council have also directed a general Index to be made of the contents of the Transactions from the year 1821 to 1830 inclusive.

Documents relating to the periods and heights of the Tides having been furnished to the Society, at the request of the Council, by favour of the Lords Commissioners of the Admiralty, who have obligingly ordered these returns to be made from the principal sea-ports of England, a Committee has been appointed for the purpose of examining and digesting them, and for printing such of the observations or results as they may deem useful.

The Committee for conducting the Meteorological Observations have been anxious to arrange a plan for insuring their accuracy, and increasing their utility. They find that standard instruments are much wanted for furnishing correct data in this department of science. This deficiency they are endeavouring to supply; and have in particular been promised the kind assistance of Mr. Daniell and Dr. Prout in superintending the construction of a standard barometer of superior accuracy, on the indications of which they expect that the utmost reliance may be placed.

The telescope, which the Council, with the advice of a Committee, had requested Mr. Barlow to construct as an experiment, on the principles stated by him in his paper in the Philosophical Transactions, is now completed, and will soon be ready for trial.

The Council have awarded one of the Copley Medals to Mr. Faraday, for his discovery of Magneto-Electricity, as explained by him in his Experimental Researches in Electricity, published in the Philosophical Transactions for the present year.

Oersted's important discovery of the influence of voltaic electricity

on a magnetic needle, was rapidly succeeded by a series of minor ones, all tending to establish the existence of an intimate connexion between magnetism and electricity. The evidence, however, of that connexion, resting, as it did, on the mutual influence of magnets and wires in which electric currents passed, and in the development or induction of magnetism by electricity, was positive on one side only; to render it conclusive, it remained to be shown that electricity could be excited by magnetism: and this, by a series of experiments as simple as they are beautiful, founded on a train of correct reasoning, Mr. Faraday has happily accomplished.

Although the Council consider that the discovery of magneto-electricity fully entitles its author to the Copley Medal, they by no means limit the value of the papers in which it is detailed to this discovery, however important. Even the preliminary facts, as they fully establish volta-electric induction, had they at the time led no further, would have been of the greatest value; but they were in hands in which they could not long remain barren, and the expectation they held out of important results was soon realized. Beyond the details of the discovery, the author rapidly but clearly establishes the laws according to which electric currents are excited by a magnet. He satisfactorily applies these laws to the explanation of a very interesting class of phenomena previously observed, namely, the reciprocal action of magnets and metals during rotation. He at the same time establishes an important distinction among bodies which had long been considered as associated by phenomena common to them all; and gives indisputable evidence of electric action due to terrestrial magnetism alone. An important addition is thus made to the facts which have long been accumulating for the solution of that most interesting problem, the magnetism of the earth.

The Council have awarded another Copley Medal to M. Poisson, for his work entitled *Nouvelle Théorie de l'Action Capillaire*. In this work a great variety of problems are solved relative to molecular attraction, some of which had not before been attempted; but the most remarkable feature of the work is, the conclusion which the author draws, namely, that the elevation and depression of liquids in capillary tubes are essentially dependent on the rapid variation of density which takes place at the surface of the fluid, and without which, according to the author, that surface would continue plane; this is at variance with the theory given in the *Mécanique Céleste*, although indeed Laplace notices this change of density at the surface, as a necessary consequence of the action of the molecules upon each other (Supp., x. livre, p. 74.) The theorems and expressions of M. Poisson do not differ in form from those of the *Mécanique Céleste*; but the constants which are involved in these equations are not expressed by the same definite integrals. No difference ensues in the consequences which are deducible from them, because the law of molecular attraction being unknown, it is impossible to arrive at the value of these constants *à priori*, or otherwise than by observation.

M. Poisson has calculated the vertical and horizontal pressures upon a solid body plunged in a fluid: the value of the latter does not

agree with that given in the *Mécanique Céleste*. According to the expression of Laplace the body might take a motion of translation: to this objections were formerly made by Dr. Young, and it will be noticed with interest that these objections are now confirmed by M. Poisson. The Council have awarded the Medal to the author, in order to testify the high sense which they entertain of the importance of the researches contained in the work in question.

The Society next proceeded to the election of the Council and Officers for the ensuing year, when the following was declared to be the list:—

President: His Royal Highness the Duke of Sussex, K.G.—*Treasurer:* John William Lubbock, Esq. M.A.—*Secretaries:* Peter Mark Roget, M.D., John George Children, Esq.—*Foreign Secretary:* Charles König, Esq.

Other Members of the Council: Francis Baily, Esq.; Captain Francis Beaufort, R.N.; Mark Isambard Brunel, Esq.; Rev. William Buckland, D.D.; Samuel Hunter Christie, Esq. M.A.; William Clift, Esq.; Rev. James Cumming, M.A.; Benjamin Gompertz, Esq.; Joseph Henry Green, Esq.; George Bellas Greenough, Esq.; William George Maton, M.D.; Roderick Impey Murchison, Esq.; William Hasledine Pepys, Esq.; Stephen Peter Rigaud, Esq. M.A.; Rev. Richard Sheepshanks, M.A.; Rev. William Whewell, M.A.

December 6, 1832.

WILLIAM GEORGE MATON, M.D., Vice-President, in the Chair.

Decimus Burton, Esq.; Charles Purton Cooper, Esq. LL.D.; and Edward Ayshford Sanford, Esq. M.P.; were elected Fellows of the Society.

December 13, 1832.

JOHN WILLIAM LUBBOCK, Esq. M.A., V.P. and Treasurer, in the Chair.

A paper was read, entitled, "On the extensive atmosphere of Mars." In a Letter to His Royal Highness the President. By Sir James South, Knt. F.R.S.

In this paper the author gives an account of a further observation which corroborates the conclusion he had stated in a former communication "On the extensive atmosphere of Mars," namely, that no indication now existed of any atmosphere being attached to that planet. A star retained its light blue colour, and its full brilliancy and comparative steadiness till the very instant of its occultation by Mars. At its emersion it was seen nearly dichotomised. The author concludes, that either some physical change has occurred in the atmosphere of that planet, or that the observations of Cassini and of Roemer were inaccurate.

A paper was also read, entitled, "On the Law which connects the various Magneto-electric Phenomena lately discovered by Dr. Faraday." By the Rev. William Ritchie, LL.D. F.R.S. Professor of Natural and Experimental Philosophy in the Royal Institution of Great Britain, and Professor of Natural Philosophy and Astronomy in the University of London.

The general principle from which the author deduces the law in question, is that of the equality of action and reaction. The application of this principle to electro-magnetism, he thinks, may be thus expressed:—since a current of voltaic electricity can, in certain circumstances, induce magnetism, magnets will, in similar circumstances, induce similar voltaic currents. He gives an account of several experiments in confirmation of the universality of this law.

A paper was then read, entitled, "An Account of an extraordinary Meteor seen at Malvern, November 12, 1832." By W. Addison, Esq. F.L.S. Communicated by W. G. Maton, M.D. V.P.R.S.

The author beheld, from the Malvern Hills, a constant succession of meteors, of various degrees of magnitude and brilliancy. The smaller ones were like those commonly called shooting stars, and left behind them, for a moment, a train of pale yellowish light. Others were much more brilliant; and notwithstanding the bright moonshine threw a strong glare upon every object, they always commenced as a small luminous point, rapidly increasing in size and splendour, shooting with great swiftness over a considerable arc, and then, suddenly disappearing, left behind them a long train of very vivid white light, which slowly changed into a pale yellow. The author witnessed this scene for upwards of an hour, although it was still going on when he left it. At one time he counted forty-eight of these meteors during the interval of five minutes.

December 20, 1832.

FRANCIS BAILY, Esq., Vice-President, in the Chair.

A paper was read, entitled, "On certain properties of Vapour." By the Rev. Dionysius Lardner, LL.D. F.R.S.

It has been generally supposed, that if a certain volume of aqueous vapour, contained in a vessel that was incapable of transmitting heat, were compressed by an exterior force into a space sufficiently small, a part of it would be restored to the liquid state. The author considers this assumption to be at variance with the doctrine of latent heat, and inconsistent with the results deduced from the experiments which have established that the absolute quantities of heat necessary to convert a given weight of water into steam, under all pressures, are sensibly equal. It follows, from this principle, that steam raised from water, under any pressure whatever, admits of indefinite compression and expansion, without returning to the liquid state. The effect of its compression will be to evolve heat and raise the temperature; that of its expansion, to absorb heat and lower the temperature: but in every state of density it will have exactly that tempera-

ture which it would have were it immediately raised from water under the pressure which it has acquired by compression or expansion. The only cause of the restoration of vapour to the liquid form is the abstraction of heat from it; and this cause will be equally operative, whatever may be the state of the vapour with respect to density: but compression alone, without such abstraction of redundant heat, can never convert any portion of vapour into a liquid. In accordance with these views, the author regards the permanent gases as vapours, containing a large quantity of redundant heat.

A paper was also read, entitled, "On the Secretion and Uses of the Bile." By B. Phillips, Esq. Communicated by W. G. Maton, M.D. V.P.R.S.

The object of the author of this paper is to establish the three following propositions; viz.

1°. That the principles of the bile pre-exist in the blood, and that the function of the liver is to separate from the blood a certain proportion of this material.

2°. That bile may be secreted as well from arterial as from venous blood.

3°. That chyle may be formed in the absence of bile.

In support of the first proposition the author adduces the analogy of other secretions, the suppression of which is followed by the appearance in the blood of the peculiar animal product which characterizes that secretion, as has been proved in the case of the urine by Prévost and Dumas, and afterwards by Vauquelin, Serullas, and Magendie. The author has confirmed the conclusion to which these physiologists have arrived, by some experiments of his own, in which, instead of extirpating the kidneys, he contented himself with tying the venal vessels. He relates two cases in which the vena portæ and hepatic artery were tied, and bile was found both in the urine and the blood.

The author, after quoting several authorities in support of his second proposition, that the liver can secrete bile, although the vena portæ be obstructed, relates two experiments which he made on dogs, by tying the vena portæ at the part before it arrives at the transverse fissure of the liver: in both cases that organ continued to secrete bile, though the quantity was small. In another dog, he tied the hepatic artery, with the effect of producing fatal peritonitis, but without any apparent change in the biliary secretion.

The arguments adduced by the author in favour of the opinion that chyle may be formed when no bile is present in the intestine, are derived from the accounts given by various authors of cases in which the ductus communis had been rendered impervious by the pressure of neighbouring tumours. In confirmation of this result, he made experiments on four dogs, and found chyle in the thoracic duct after he had tied the ductus communis close to the duodenum.

The author concludes, from these and other facts, that the secretion of bile is intended to serve some other purpose than that of contributing to the formation of chyle.

A paper was communicated to the Society, entitled, "Experimental Researches on Electricity, Third Series," by Michael Faraday, Esq. D.C.L. F.R.S. M.R.I., the reading of which was deferred to the next Meeting.

The Society then adjourned over the Christmas Vacation, to meet again on the 10th of January.

January 10, 1833.

JOHN WILLIAM LUBBOCK, Esq. M.A., V.P. and Treasurer, in the Chair.

The reading of Mr. Faraday's paper, communicated at the last Meeting, and entitled, "Experimental Researches on Electricity, Third Series," was commenced.

January 17, 1833.

MARK ISAMBARD BRUNEL, Esq., Vice-President, in the Chair.

The reading of Mr. Faraday's paper was resumed and concluded.

The object of the inquiries of which an account is given in the present paper, is to establish the identity of the electricities derived from different sources. The author was induced to investigate this subject, because doubts have been frequently expressed as to the accuracy of some experiments from which the identity of common and voltaic electricities is inferred: and distinctions have been drawn between them, as if they were different forms and modifications of one common power. In order to examine the question in all its bearings, he arranges the phenomena under two general heads; namely, those arising from electricity in a state of equilibrium, or *tension*, as it has been called; and those which are the consequence of its *motion*, when that equilibrium has been destroyed. The visible effects of electricity of tension are attractions or repulsions at sensible distances; those of electricity in motion are the evolution of heat, the production of magnetism, chemical decompositions, physiological changes, and, lastly, the evolution of light in the form of a spark. The author proves, by experiments, that every one of these phænomena takes place from the operation either of ordinary or of voltaic electricity; the degree in which they are produced depending on the different circumstances of quantity, of intensity, and of velocity, attendant on the different modes in which electricity has been excited and supplied. Thus no difference was found to exist in the mode in which a Leyden battery charged with ordinary electricity, and a voltaic battery, were discharged, when the comparison was made by means of fine points, nicely arranged and approximated, either through air of the ordinary temperature, or through heated air, such as the flame of a spirit-lamp, interposed between the points.

By the term *current*, the author designates any progressive change, of whatever nature it may be, in the electric state, whether consisting

in the motion of one electric fluid in a particular direction, or of two fluids in contrary directions: and by the term *arrangement*, he understands a local adjustment of particles, or fluids, or forces, not progressive.

By *ordinary electricity*, he understands that which can be obtained from the common electrical machine, or from the atmosphere, or by pressure, or cleavage of crystals, or similar mechanical operations; its character being that of great intensity, and the exertion of attractive and repulsive forces, not merely at small but also at considerable distances. The parallel between voltaic and ordinary electricity is then pursued by the production of evidence that those attractions and repulsions, which were thought to characterize the latter, are exhibited also by the former; and that, on the other hand, ordinary electricity, when in motion, gives rise to an increase of temperature, to magnetic phenomena, to chemical decompositions, to physiological impressions, and to luminous appearances, precisely of the same kind as those which had been supposed to be the peculiar effects of voltaic electricity. The experiments of Mr. Colladon, which seemed to show that a stream of common electricity has power to produce the deflexion of a magnet,—a conclusion which has hitherto rested on the single testimony of that experimentalist,—have been repeated and extended by Mr. Faraday, who completely confirms their accuracy, and the truth of the result that had been obtained from them. The author succeeded in making common electricity assume more of the characters of voltaic electricity, by availing himself of the retarding power of bad conductors interposed in the electric circuit. In this way he easily succeeded in obtaining the same decisive evidence of chemical action by common electricity as Dr. Wollaston had done in his experiment. But Mr. Faraday considers the experiment in which water is decomposed by this power, as affording no proof of electro-chemical agency; because, as Dr. Wollaston had pointed out, both oxygen and hydrogen are evolved at each of the points of the interrupted circuit, and not separately at the respective poles. The author regards the amount of electro-chemical decomposition as being proportional, not to the *intensity*, but to the *quantity* of electricity transmitted. It is not effected by electricity passed from the machine in sparks, although these tend to decompose water into its constituent elements. Some experiments of Bonijol on the decomposition of water by atmospherical electricity, are commented on by the author, who considers them as analogous to the experiment of Dr. Wollaston already referred to. Mr. Faraday also makes some remarks upon Mr. Barry's paper in the Philosophical Transactions for 1831, and suggests doubts of the soundness of the inferences he draws from his experiments.

The author then proceeds to examine the electrical phenomena elicited by magneto-electricity, and shows that, as far as they have been observed, they coincide with those of voltaic electricity, and, consequently, are referrible to the same agency. The only effects that have not been yet obtained are chemical decompositions. The quantities of thermo-electricity that can be elicited in ordinary cases are

too small to produce any effects but those of magnetism, and also muscular contractions in the limbs of frogs. The animal electricity of the torpedo produces most of the effects of voltaic electricity, excepting the evolution of heat and light. The general conclusion deduced by the author from these researches is, that electricity, whatever be its source, is perfectly identical in its nature.

In the concluding chapter of the present paper, the author endeavours to establish some relation by measure between common and voltaic electricity. He shows, by experiment, that whenever the same *absolute quantity* of electricity, whatever be its *intensity*, passes through the galvanometer, the deflecting force exerted upon the magnetic needle is invariably the same. Hence this deflecting force may be taken as the measure of the absolute quantity of transmitted electricity; a principle which establishes the value of the galvanometer as an instrument of measurement in all cases of electricity in motion. The power of chemical decomposition he finds to be also directly as the quantity of transmitted electricity.

January 24, 1833.

The Rev. WILLIAM BUCKLAND, D.D., Vice-President, in the Chair.

A paper was read, entitled, "Magnetical Experiments, made principally in the South of Europe and Asia Minor, during the years 1827 and 1832." By the Rev. George Fisher, M.A. F.R.S.

This paper is divided into five sections. The first gives an account of a series of experiments made with a view to determine the relative intensities of the forces soliciting a horizontal magnetic needle, and also the forces in the direction of the dipping needle, at London, Lisbon, and Gibraltar; premising a minute description of the apparatus employed, and a circumstantial statement of the methods used for conducting the investigation.

The second section gives the details and results of similar experiments made at London, Malta, Syracuse, Catania, Messina, Naples, Baia, Constantinople, Egina, and Athens; and also on the plain of Troy, and at Vourla in Asia Minor.

The third section contains an account of experiments on the diurnal variation in the intensity of the magnetic force soliciting a horizontal needle in the island of Malta.

In the fourth section, experiments are related on the diurnal variation of the magnetic needle suspended horizontally at Malta.

The fifth section is occupied by an account of the results of similar experiments made on the bases and edges of the craters of Vesuvius and Ætna; and also on Gibraltar rock, and the neutral ground below: from which it appears, that the forces soliciting both the horizontal needle and that in the position of the dip, were considerably greater on the elevated than on the lower situations.

From the whole of the observations made in different parts of the Mediterranean, and contained in this paper, it appears that great

irregularity exists in the numerical results; but whether these arise from irregularities in the distribution of the terrestrial magnetism, or from any active agency of a volcanic nature, or other cause, the author does not venture to decide.

January 31, 1833.

JOHN WILLIAM LUBBOCK, Esq. M.A., V.P. and Treasurer, in the Chair.

A paper was read, entitled, "An experimental Inquiry into the Treatment of Tic Douloureux." By W. R. Whatton, Esq. F.S.A. M.R.C.S. Communicated by P. M. Roget, M.D. Sec. R.S.

The author, after giving a brief account of the history of neuralgia facialis, and of the opinions that have been entertained of its nature, states the results of his trials of morphia applied to the skin, when denuded of cuticle by the previous application of a blister. The form he usually employs is that of a cerate, composed of eight grains of the acetate of morphia, finely powdered, to an ounce of simple ointment or lard, one dram of which is applied every eight hours. After a few weeks the proportion of the acetate is doubled, and occasionally, in very severe cases, a cerate, containing twenty or twenty-four grains in the ounce, was used. He relates a number of cases in which this treatment was perfectly successful in curing the disease, even when it had been of long standing, and had resisted every other mode of treatment.

February 7, 1833.

WILLIAM GEORGE MATON, M.D., Vice-President, in the Chair.

The Very Rev. George Chandler, D.D., Dean of Chichester; Woronzow Greig, Esq. M.A.; and the Rev. Frederick Nolan, LL.D.; were elected Fellows of the Society.

A paper was read, entitled, "On the relation which subsists between the Nervous and Muscular Systems in the more perfect Animals, and the nature of the Influence by which it is maintained." By A. P. W. Philip, M.D., F.R.S. L. & E.

The author, after referring to his former papers which have at different times been read to the Royal Society, and published in their Transactions, is led to view the brain and spinal marrow as the only active parts of the nervous system; the nerves, whether belonging to the class of cerebral or ganglionic, together with their plexuses and ganglions, serving only as the means of conveying and combining the various parts of the former organs, and therefore being passive with reference to their functions. This view of the subject is directly opposed to that which has been adopted by many physiologists, who consider these ganglions as the sources, and not the mere vehicles, of nervous influence. In order to determine this point, the author made the following experiment on an animal that had been pithed

so as to destroy its sensibility, while the action of the heart continued. Under these circumstances, he applied mechanical irritation, and also various chemical agents, to the ganglions and plexuses of the ganglionic nerves, and found that the heart continued to beat with the same regularity as before, and with the same frequency of pulsation. From these and other observations, the author concludes that the ganglionic system of nerves, with their plexuses and ganglions, performs the office of combining the influence of every part of the brain and spinal marrow, and of bestowing it on the muscles of involuntary motion, these muscles being subservient to those functions of life which require that combined influence; that the manner in which the influence of these organs affects the muscular fibre is not essentially different from that of other stimulants and sedatives; and that this influence is not an agent peculiar to the nervous system, but is capable of existing elsewhere, and is consequently not a vital power, properly so called; a conclusion which appears to him to be confirmed from the circumstance that galvanism is capable of performing all its functions. Hence he infers that the brain and spinal marrow, far from bestowing on the muscular fibre its peculiar power, only supplies an inanimate agent, which, like all other such agents, capable of affecting it, acts on it either as a stimulant or sedative, according to the degree in which it is applied, and is identical with the galvanic influence.

February 14, 1833.

The Rev. WILLIAM BUCKLAND, D.D., Vice-President, in the Chair.

A paper was read, entitled, "On the Existence of four distinct Hearts, having regular Pulsations, connected with the Lymphatic System, in certain Amphibious Animals." By John Müller, M.D., Professor of Physiology in the University of Bonn. Communicated by Leonard Horner, Esq., F.R.S.

The author had long ago observed, that, in frogs, there exists, immediately under the skin, large spaces containing lymph, whence it can be readily collected by making incisions through the skin. These receptacles for lymph are larger in the frog than in the other amphibia: but all the animals of this class appear, from the observations of the author, to be also provided with remarkable pulsating organs, which propel the lymph in the lymphatic vessels, in the same way as the heart propels the blood circulating in the arterial system. In the frog, two of these lymphatic hearts are situated behind the joint of the hip, and immediately underneath the skin. Their contractions are performed with regularity, and may be seen through the skin; but they are not synchronous either with the motions of the heart, or with those of the lungs, and they continue after the removal of the heart, and even after the dismemberment of the animal. The pulsations of these two organs on the right and left side are not performed at the same time, but often alternate at irregular intervals.

The author proceeds to trace the connexions of these cavities with the lymphatic vessels in the neighbourhood, and with one another: and it appears from his researches, that the lymph of the hinder extremities, as well as that of the posterior part of the abdomen, is conveyed by means of these hearts into the trunk of the crural veins. He also gives a description of the posterior part of the venous system of the frog, noticing particularly the large transverse anastomosis between the sciatic and the crural veins, which joins the anterior median vein of the abdomen, and conducts the blood partly into the vena portæ, and partly into the renal veins.

Professor Müller has likewise discovered two anterior lymphatic hearts in the frog; a discovery to which he was led by some observations of Dr. Marshall Hall, who stated that he had seen in that animal an artery pulsate after the removal of the heart. These anterior lymphatic hearts lie on each side upon the great transverse process of the third vertebra, immediately under the posterior end of the scapula, and they are nearly as large as the posterior hearts. They receive the lymph of the anterior parts of the body, and probably also that of the intestinal canal, in order to transmit it into contiguous veins, which pour their contents into the jugular vein. The author has discovered similar organs in the toad, the salamander, and the green lizard; and is of opinion that they exist in all the amphibia.

The following announcement was made from the Chair:—

"His Royal Highness the President has received from Professor Gauss the abstract of a paper read by him at the meeting of the Royal Society at Göttingen, on the 15th of December last, entitled '*Intensitas vis magneticæ terrestris ad mensuram absolutam revocata.*' Mr. Gauss's views possessing considerable interest, His Royal Highness is desirous that they should be made known to the Fellows of the Royal Society; but as the original paper will not be printed for many months, and the abstract which appeared in the *Göttingische gelehrte Anzeigen* is in a language not generally understood in this country, His Royal Highness has requested your Foreign Secretary to translate it; and I am commanded to desire your Secretary to read the same to the present meeting.

"In deviating thus far from the usual routine of the business of the Royal Society, His Royal Highness is actuated by a wish to promote the reciprocal and early communication of new and important discoveries and views in science, between our own and the other Societies of Europe, devoted, like this, to '*the improvement of natural knowledge.*'

"Communications of this nature, however, cannot of course be admitted into your Transactions; but the publication, from time to time, of your Proceedings, affords a happy means of giving them general circulation; and thus the rapid propagation of much valuable information will be effected, which otherwise, if not absolutely lost to us, would, at least, long remain unknown to the British scientific public."

The following is the abstract of Professor Gauss's Memoir :—

Of the three elements which determine the manifestation of terrestrial magnetism in a given place, viz. Declination, Inclination, and Intensity, the first soonest engaged the attention of philosophers, the second much later, and the third has only at a very recent period become an object of investigation and experiment. This progressive interest is chiefly to be accounted for by the circumstance, that while the variation of the compass offered the greatest interest, as applied to the purposes of navigation and geodesic operations, the dip was looked upon as more nearly allied to it than was the intensity of terrestrial magnetism. To the natural philosopher, those three elements are absolutely of the same import, inasmuch as our knowledge of the general system of terrestrial magnetism will ever remain imperfect, until an equal share of attention has been bestowed on its separate branches.

For the first light thrown upon this subject we are indebted to the Baron Humboldt, whose attention was particularly directed to it during all his travels, and who has furnished a considerable series of observations, from which the gradual increase of this intensity, from the magnetic equator of the earth towards the magnetic poles, has been deduced. Many observers have since followed the footsteps of that great naturalist; and almost every part of the world to which, in recent times, travellers have penetrated, has furnished its quota of materials, from which already Hansteen (to whom this branch of philosophical inquiry is under great obligation) has been enabled to attempt the construction of an iso-dynamical chart.

The mode adopted in all these observations consists in disturbing the equilibrium of one and the same magnetic needle in places the comparative intensity at which is to be determined, and in exactly measuring the duration of its oscillations. This duration is indeed, *cæteris paribus*, dependent on the magnitude of the arc; but in such a manner, that however small the arc becomes, it still approaches a determined limit, loosely called the duration, and to which, the arc of oscillation being known, the really observed duration may easily be reduced. The intensity of terrestrial magnetism is thus inversely proportional to the square of the duration of oscillation of the same needle, or directly so to the square of the number of oscillations in a given time; and the result relates to the whole force, or to the horizontal portion of it, according as the needle has been caused to vibrate, in the plane of the magnetic meridian, round a horizontal axis, or, in a horizontal plane, round a vertical axis.

It is evident that the admissibility of this method entirely rests on the assumption of the unchanged magnetic state of the needle employed. If a properly-magnetized and carefully-preserved needle of good hardened steel be made use of for the experiments, and these do not take up too long a space of time, the danger to be apprehended from such alteration may not, indeed, be considerable; and the observer may rest the more satisfied in this respect, if, on returning to the first place, he find the time of the vibration to be the same; but experience teaches us that this result cannot by any means be calculated

upon ; neither can it be denied, that in resorting to such a proof we are only reasoning in a circle. It was known indeed, long ago, that both the declination and inclination in the same place are far from being invariable; that both of them, in the course of time, undergo very considerable progressive variations, independently of those periodical ones by which the nicety of observation is affected in different seasons and parts of the day. It is, therefore, no matter of doubt that the intensity of terrestrial magnetism must likewise be subject to them ; indeed, the periodical diurnal variations are clearly perceptible in delicate observations. Hence, even if, after a considerable lapse of time, the same time of vibration is again observable in a given place, we are not, on that account, warranted in ascribing this circumstance to anything but a casual compensation of the variations which the intensity of the magnetism of the earth in that place, and the magnetic state of the needle itself, may have experienced during that interval. But even allowing the certainty of the comparative method to be only diminished to a certain degree, not entirely annulled, provided too long a space of time do not intervene, that mode, at all events, becomes entirely useless in cases where it is required to ascertain what changes the intensity of terrestrial magnetic force undergoes in a given place during a very long interval. This question, of considerable interest in a scientific point of view, must, therefore, remain unanswered until the merely comparative method shall be superseded by one which reduces the intensity of terrestrial magnetism to unities perfectly determined and manifest, and entirely independent of the individual nature of the needles employed in the experiments.

It is not difficult to lay down the theoretical principles on which such an independent method is to be founded. The time of oscillation of a given needle depends on three quantities ; namely, the intensity of the terrestrial magnetism, the static momentum of the free magnetism in the needle, and the momentum of the inertia of this needle. The last of them may readily be ascertained by suitable methods ; and thus, from the observed duration of the oscillation, is deduced, not the quantity of the intensity of the terrestrial magnetism, but the product of this quantity into the static momentum of the free magnetism in the needle. But it is impossible to separate these two factors from one another, unless observations of quite a different kind be superadded, that involve a different combination of them ; and this end is attained by the use of a second needle, which, in order to ascertain the ratio of these forces, is subjected both to the influence of the magnetism of the earth and to that of the first needle. These two effects do, indeed, partly depend on the magnetic state of the second needle ; but, by suitably conducting the experiments, the observer may eliminate that state, inasmuch as the *ratio* of both forces becomes the more independent of it, the greater the distance of the two needles from one another is assumed. Here, however, it is obviously necessary, at the same time, to consider the position relative to the magnetic meridian, of the magnetic axes of both needles, and of that of the straight line connecting their centres, as also the magnetic state of the first needle ; all which cannot be subjected to

computation unless we know the law of the force exerted on each other by two elements of free magnetism, or, in other words, with which, according as they are of the same or different denominations, they repel or attract each other. Tobias Mayer had already conjectured this law to be the same with that of general gravitation, i. e. that the force is in the inverse ratio of the square of the distance. Coulomb and Hansteen have endeavoured experimentally to confirm this conjecture; and the fact is now completely established by the experiments detailed in Professor Gauss's forthcoming memoir. This law, however, only relates to the elementary effect; for the computation of the total effect of a magnetic body on another, as soon as the nature of the distribution of free magnetism in these bodies is accurately known, becomes a problem purely mathematical, and consequently remains dependent on their casual individual nature; but the greater the distance, the less the influence of this individuality becomes; and if the distance be very great, we may, *cæteris paribus*, assume (as indeed follows from the above principle,) the total effect to be inversely proportional to the cube of the distance. The product of this cube into the fraction which expresses the ratio of the effect of the first needle, and of the terrestrial magnetism on the second needle, will therefore, as the distances continually increase, tend to a determined limit. A proper combination of observations at several judiciously selected distances will, being mathematically treated, make us acquainted with that limit, from which may be deduced the *ratio* of those two quantities the product of which was derived from the observed times of vibration. The combination of both results will then obviously give those two quantities themselves.

The experiments for comparing the effects of the magnetism of the earth, and of the first needle on the second, suspended by a thread, may be conducted in two different ways; inasmuch as the latter may be observed either in a state of motion or of rest. The former is best effected by placing the first needle in the magnetic meridian of the second, whereby the time of a vibration of the latter is either increased or diminished, according as poles of the same or of different names are opposed to each other. The comparison of the time of vibration thus changed, with that occasioned by terrestrial magnetism alone, or rather, the comparison of an increased with a diminished one (under opposite directions of the first needle), will then readily lead to the ratio sought. The second mode is that of placing the first needle in such a manner that the direction of its influence on the second makes an angle with the magnetic terrestrial meridian; when the angle of deviation from the meridian, in a state of equilibrium, will equally lead to the knowledge of the ratio sought. And here, too, it is more advantageous to compare with each other two opposite deviations, under opposite positions of the first needle. The most advantageous position of this needle is along a straight line drawn through the middle of the second and perpendicular to the magnetic meridian. The first mode agrees upon the whole with that proposed some years ago by Poisson; but the experiments, as far as we have any record of them, made by some natural philosophers with a view to apply that mode, have either

entirely failed, or their results can at best be considered only as imperfect approximations.

Professor Gauss, who has made frequent trials of both those modes of proceeding, is satisfied that the second is, on many accounts, far preferable to the first.

The real difficulty consists in this, that other elements depending on the individual nature of the needles, enter, as well as the value of the limit, into the influences observed. That effect is represented by a series which proceeds by the negative powers of the distance, beginning from the third; where, however, the following terms become more considerable as the distance is smaller. Now those following terms are to be eliminated by means of several observations; but a slight acquaintance with the theory of elimination easily convinces us that unavoidable errors of observation will never fail to endanger the exactness of the results, as the number of co-efficients to be eliminated is greater; so that their number need not be very considerable to render the results of computation entirely useless. No precision, therefore, in the results can be expected, unless such considerable distances are employed as will make the series rapidly converge, and a few terms of it suffice. But in this case the effects themselves are too small to be determined with exactness by our present means of observation; and thus the ill success of the experiments hitherto made is readily explained.

However easy, therefore, in theory the methods of reducing the intensity of terrestrial magnetism to absolute unities may appear, yet their application will ever remain precarious until magnetic observations have attained to a much higher degree of precision than they have hitherto possessed. It is with this view that Professor Gauss has followed up several ideas long ago entertained by him relative to the improvement of our means of observing; confidently expecting that magnetic observations will, ere long, be carried to a degree of perfection nearly, if not altogether, equal to that of the most delicate astronomical observations. The expectation has been answered by the result. Two apparatus fitted up in the observatory of Göttingen, and which have been employed for making the observations, of which several are given in his memoir, leave nothing to desire but a suitable locality completely secured from the influence of iron and currents of air.

The following short abstract from the detailed description of the two apparatus and their effect, given in the memoir itself, will no doubt be acceptable to naturalists interested in this kind of research.

Professor Gauss has generally employed needles (if prismatic bars of such strength may be designated by that name) of nearly a foot in length, weighing each about one pound. They are suspended by an untwisted thread of $2\frac{1}{2}$ feet in length, composed of thirty-two threads of raw silk, and thus able to carry even double that weight without breaking. The upper end of the thread is tortile, and the degree of torsion is measured by means of a divided circle. To the south or the north end of the needle (according as the locality renders either the one or the other more convenient), a plane mirror is fixed, the sur-

face of which, by means of two adjusting screws, may be placed perpendicular to the axis of the needle; but scrupulous attention need not be paid to this adjustment, as any deviation may most exactly be measured by the observations themselves, and taken into account as errors in collimation. The needle thus balanced is enclosed in a wooden cylindrical box, which, besides the small aperture in the lid for the passage of the thread, has a larger one in the side, which is rather higher and wider than the mirror already mentioned.

Opposite to the mirror, a theodolite is placed, the vertical axis of which is in the same magnetic meridian with the thread of suspension, and at a distance from it of about sixteen Parisian feet. The optical axis of the telescope is placed rather higher than the needle, and inclined in the vertical plane of the magnetic meridian, so as to be directed towards the centre of the mirror on the needle.

To the stand of the theodolite is fixed a horizontal scale of four feet in length, divided into single millimetres: it makes a right angle with the magnetic meridian. That point of the scale which is situated in the same vertical plane with the optical axis of the telescope, and which, for the sake of brevity, may be denominated the zero point, is marked out by a fine thread of gold depending from the middle of the object-glass, and charged with a weight. The scale is fixed at such a height that the image of a portion of it is seen in the mirror through the telescope, the eye-glass of which is adjusted accordingly. At the opposite side from the needle, in the same vertical plane, and at a distance from the telescope equal to that of the image, a mark is fixed, serving every instant to ascertain the unchanged position of the theodolite.

It is obvious, that if all these conditions be fulfilled, the image of the zero point on the scale will appear exactly on the optical axis of the telescope, and that, so far as an object of known azimuth is visible at the place of the theodolite, we may, by means of this instrument, immediately find the absolute magnetic declination. If, on the other hand, those conditions are only partially fulfilled, then, generally speaking, the image, not of the zero point, but that of another point of the scale, will appear on the optical axis; and if the horizontal distance of the scale from the mirror have been measured with exactness, it will be easy to reduce the amount of the divisions of the scale to the corresponding angle, and thus to correct the result first obtained. By turning the needle in the stirrup (so that the upper surface becomes the lower), the amount of the error of collimation of the mirror may be ascertained with great ease and precision. In both the apparatus, one part or division of the scale is equal to nearly twenty-two seconds; an interval which even the least practised eye may easily subdivide into ten parts.

By this mode of operating, therefore, the direction of the needle and its variations are determined with the greatest possible precision. It is by no means necessary always to wait till it is at rest; as the two elongations to the right and the left may be observed with great accuracy, and their combination, properly managed, will indicate the corresponding point of rest with equal precision. During the antemeri-

dional hours, when the daily variation is most rapid, this may be followed almost from one minute of time to the other.

Of equal importance is this mode of proceeding for observing the duration of the vibrations. The passage of the vertical thread in the telescope before a fixed point of the scale (properly speaking, the reverse is the case), may, even if the whole deviation only amount to a few minutes, be observed with such a degree of precision as never to leave any uncertainty amounting to the tenth of a second in time. The considerable duration of a vibration (about 14 seconds in the most intensely magnetized needles), and the slow degrees by which the arc decreases, are productive of other important advantages: only a few vibrations are required to enable us to determine the time of one vibration with such accuracy, that, though the needle be left to itself for one or even several hours, no doubt will remain on the mind of the observer as to the number of oscillations performed during the interval of his absence. We may commence with vibrations so small (such, for instance, as those with which we generally leave off,) that the reduction to infinitely small vibrations becomes almost imperceptible; and yet, after an interval of six and more hours, the vibrations are still found sufficiently great to admit of having their beginnings observed with all requisite precision.

In cases where anomalies still appear in the observations (which, however will prove so trifling, that with the common means they would have been altogether imperceptible,) they are solely to be ascribed to the current of air which, in the locality where the experiments were made, could not be altogether avoided. To remedy this inconvenience the aperture of the box might be closed by a plane glass; but none perfectly true was within the author's reach, neither could it have been made use of without an inconvenient loss of light.

To the enumerated advantages of this method another may be added, which is, that the observer constantly remains at a great distance from the needle, while in the old mode of proceeding his proximity to it was unavoidable; so that, even if enclosed in a glazed case, it was exposed to the disturbing influence which might be exerted upon it by the warmth of the body, or that of the lamp, by the iron or even the brass which the experimenter might happen to carry about him.

The advantages of stout heavy needles over those of diminutive size, which have been made use of for most magnetical observations, particularly those relating to the time of vibration, are dwelt upon by Professor Gauss; he has since successfully employed one weighing upwards of two pounds, and expresses his conviction, that if needles of from four to six pounds in weight were used, on which slight currents of air would cease to exert any perceptible influence, magnetic observations might attain an exactness and precision unsurpassed by the most delicate astronomical observations. Much stronger threads would indeed be required for suspension, the torsion of which would produce greater reaction; but whatever the strength of the thread may be, the force of torsion must always, and may without any difficulty, be taken into account with the greatest exactness.

The two apparatus may likewise be made use of for another purpose, which, though not immediately connected with the principal subject of the memoir, may still be adverted to in this place. They are the most sensible and convenient galvanometers both for the strongest and weakest energies of the galvanic current. To measure the strongest, it is only required to bring the conducting wire single, and at a considerable distance (at least several feet), into the magnetic meridian below or above the needle; for very weak energies a multiplier is wound round the box containing the needle. Some of the experiments were made with a multiplier of 68 circumvolutions, producing a length of wire equal to 300 feet. No pair of large plates is requisite: a pair of small buttons, or even simply the ends of two different metallic wires dipped in acidulated water, produce a current indicated by the movement of the image along many hundred parts of the scale; but on using a pair of plates of very moderate dimensions, the image of the whole scale, as soon as the circuit is completed, is seen rapidly to dart through the field of vision of the telescope. It is obvious that by this method the measurement of galvanic forces may be conducted with a degree of ease and precision unattainable by the hitherto employed laborious modes by means of observed times of vibration; and it is literally true that by it we are enabled to follow from second to second the gradual decrease of the intensity of a galvanic current, which, it is well known, is more rapid in the beginning. If, in addition, instead of the single, a double (astatic) needle is used, no degree of electro-magnetic energy will be found too small to admit of being still measured with the utmost precision. Here, therefore, a wide field is opened to the naturalist for most interesting investigation.

Not a small portion of this unpublished memoir of Prof. Gauss is taken up by the developement of the mathematical theory; and also by various methods peculiar to the author, such as the determination of the momentum of inertia of the vibrating needle, independently of the assumption of a regular figure; by his experiments with a view to establish the above-mentioned fundamental law for the magnetic effects; and, finally, by the details of the experiments to determine the value of the intensity of terrestrial magnetism, of which last the following may be given as the results, as far as they relate to the intensity of the horizontal part of that force.

I. May 21	1·7820
II. May 24	1·7694
III. June 4	1·7713
IV. June 24—28 ..	1·7625
V. July 23, 24 ..	1·7826
VI. July 25, 26 ..	1·7845
VII. Sept. 9	1·7764
VIII. Sept. 18	1·7821
IX. Sept. 27	1·7965
X. Oct. 15	1·7860

For unities, the millimetre, the milligramme, and the second in time

have been adopted. The manner in which the measurement of the intensity has been determined by them cannot here be specified: the numbers, however, remain the same, provided the unity of space, and that of weight (properly speaking, unity of masses), are changed in the same proportion. These experiments vary partly with regard to the greater or less degree of care with which they were conducted, partly with regard to the places in which they were made, and to the needles employed.

The experiments VII, VIII, IX, were in every respect performed with all the precision which the apparatus in the present state admits of, and the distances were measured with microscopic exactness. In experiments IV, V, VI, X, some operations have been performed with rather less care; and the three first experiments are still less perfect in this respect.

The needles employed in the first eight experiments were not indeed the same, but they were nearly alike in size and weight (the latter between 400 and 440 grammes); the principal needle in experiment X. weighs 1062 grammes; experiment IX. on the other hand is made, with a much smaller needle (weight 55 grammes), merely for the sake of ascertaining the degree of precision, which, all other precautionary means being alike, may be attained in using a needle of such small dimensions: the result of this experiment is therefore much less to be depended upon.

Experiments VII. to X. were made in one and the same place in the observatory; the preceding ones in other places in the same observatory, and in apartments of the author's dwelling-house. No perfectly pure results therefore could be derived from these latter experiments, inasmuch as the iron in those localities, and particularly in the observatory, becoming itself magnetic by the magnetism of the earth, would necessarily react upon the needle, and confound its influence with that of the terrestrial magnetism. Such places, indeed, were uniformly chosen in which neither fixed nor moveable masses of iron were near; nevertheless, even the more distant ones may not have been altogether without their effect upon the operations. However, on casting a look over the different results, it appears probable, that in no one of those localities, the modification of the terrestrial magnetism produced by extraneous influence exceeds the hundredth part of the whole. But results commensurate to the precision belonging to this mode can only be expected in a locality entirely free from the influence of iron.

In order to obtain the intensity of the *whole* force of the terrestrial magnetism, the numbers found are to be multiplied by the secant of the inclination. Mr. Gauss intends at a future period also to treat this element according to peculiar methods; in the mean time he merely mentions that on June the 23rd he has found 68° 22′ 52″ with the inclinatorium of the University collection of instruments,—a result which, as the observation was made in the observatory, and therefore not without the reach of local interference, may possibly require to be rectified by other observations.